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EDITORIAL.

The following contribution to the discussion of the subject of generic types has been sent to us by Mr. Louis B. Prout, of London, England, and refers to the discussion in the June, 1904, number of this Journal:

“Permit me to suggest that if the editor’s views and those of Prof. J. B. Smith, as there set forth, be combined, with the elimination of all elements of personal preference, etc. (which you will agree with me, are the causes of instability and of discord), we shall have an almost perfect system, and one that — as it would work well-nigh automatically — we should all do well to adopt. Evidently adherence to an original spelling would be undoubtedly ‘automatic’; I rejoice to see that you very nearly advocate this, and if I read you aright you are not even *hopelessly* antagonistic to the non-Latin ‘w’ in dedicatory names. Why should *Walkeria* or *Walsinghamia* be disguised by an initial U or V? Surely we in England must not advocate such a proceeding, when our national coinage is perpetuating the barbarous ‘Edwardus’! As to the ‘k,’ you are of course right, and Sir George Hampson’s change to ‘c’ indefensible. Better reject dedicatory names *in toto* than mutilate them beyond recognition.

“The inconsistency of demanding *verbal* definition you have admirably exposed, as also previously in *Trans. Am. ent. soc.*, xxiv, 6. Rothschild and Jordan have made a pretty business of it in

their otherwise masterly 'Revision of the Sphingidæ.' They first reject all genera that are not diagnosed; next they reject all diagnoses prior to their own (*e. g.*, citing as 'type' a species which absolutely contradicts the old diagnosis); why are they not logical enough to end by rejecting all generic *names* 'prior to their own'?

"This brings me to the last, and most important point, the defence, by certain lepidopterists, of the illegal practice of making the first species of a genus the 'type,' irrespective of historical action. I know of no code which permits such a course, and am at a loss to know what right we lepidopterists have to be 'a law unto ourselves' in so vital a matter; the general zoological rules must be our guide. Of course, if we are willing to trample on diagnoses and on common sense, and to make *prasinana* the type of *Tortrix* and so on, the method will be automatic, and therefore in a sense useful; but if we apply it reasonably—as even Sir George Hampson advocates—it is no more automatic than the legal method, properly understood and applied. Perhaps you have overlooked the fact that the different results arrived at by different workers professing to follow the 'elimination' method are mainly due to their having tried to follow the indefensible and impossible applications of it which have unfortunately stultified the results in Scudder's otherwise magnificent 'Historical sketch'; *i. e.*, they have allowed one name, independently erected, to 'restrict' another—whereof the second author had probably never even heard!—have forbidden an author or reviser to fix as type of one genus a species which has earlier been made (or which even now becomes, on their arbitrary methods) the type of another, and have brought in other extraneous elements which have resulted, as Sir George Hampson has so well said, in a 'reductio ad absurdum.' If the history of each name were traced independently and types fixed in accordance, the matter would be greatly simplified. Compare Walsingham and Durrant's 'Merton Rules,' No. 44: 'He who first restricts a genus under its own name limits the possible type,' etc. There is nothing 'absurd' in this, quite the reverse; for it recognizes and respects an intention to revise antecedent work, and fulfils the requirements of the 'British Association' and other codes. Theoretically, no author ought to revise nomenclature without knowing his literature (of course mere faunistic lists can be ignored as they have no restrictive influence); but even if, as you suggest, some reference were overlooked by the monographer, it would not dislocate an entire

cantention of names, as on the Scudder system, but, at the worst, only the one, or ones, immediately involved. A moment's thought will show that, given the literature of a certain name, the type can be decided as automatically, and almost as quickly as, and certainly far more logically than, by the arbitrary selection of the first species—a method which I cannot agree with you is at all likely to obtain in the long run, in face of the strong arguments which were adduced against it by nearly all the authorities who took part in the Sir George Hampson Nomenclature Correspondence (Proc. Internat. Congr. Zool., App. A, Cambridge, 1898) and of the fact that in this matter we ought to work harmoniously with other zoölogists, who can hardly be expected to consent to a course which would bring about such disastrous results in their particular departments.”

LOUIS B. PROUT.

LONDON, N. E., 18 Nov., 1904.

Our correspondent advocates what Rothschild and Jordan call the “First method of restriction,” or the method of nomination of types.* We admit that we have not seen this method fairly tried, though we had thought the same objections applicable to it as to the other “historical method,” the second method of restriction of Rothschild and Jordan, or the method of residues. These methods are sometimes thought to be similar or parts of one method, but, as Rothschild and Jordan say: “As the first and second methods are opposed to one another, differing nearly always in the results attained, we reject them both.” Of course it does not necessarily follow that a method should be rejected because opposed to another; that might prove it the right one. But we are pleased to see that Mr. Prout condemns the method of residues. This is the method heretofore used by Lepidopterists from Scudder to Kirby. It must be abandoned. What method shall we substitute? Mr. Prout urges conformity with other zoölogists; but we have yet to see a code of rules that clearly covers the points of the present subject.

* Called by Kirkaldy “The historical method” and advocated by him. (Proc. ent. soc. Wash., vii, 1905.)